an array of facts as a trained geologist can gather in the field, and by boldly announcing the conclusions to which the study of these facts has led him. But much more may be made of them than he has yet given us. And we trust he may be encouraged to continue the investigation he has so well begun.

ARCH. GEIKIE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Marine Biological Association

WILL you allow me space to ask all naturalists and lovers of science who intend to become members of the above Association to send their names and subscriptions *without delay* to Mr. Frank Crisp, 6, Old Jewry, London, E.C. The subscription is one guinea annually, or fifteen guineas for life membership. It is highly desirable that intending members should at once enrol themselves, since the first meeting of the Association for the election of officers and council for the year 1884–85, and for the ratification of by-laws, will be held in London at the end of this month, when Prof. Huxley will be nominated as President. Donations, whether large or small, are earnestly solicited. Those who are interested in the natural history of marine plants and animals, and who foresee the immense help to this study which a well-equipped laboratory will afford, are begged not only to give some pecuniary aid to the present enterprise, but to constitute themselves agents of the Association and to do their best to persuade others to contribute to the fund required for building the first biological laboratory on the English coast. It is only by hearty and earnest support of this kind that our object can be realised.

I may add that several naturalists have contributed each 100*l.* to the Association, others 25*l.*, and others less, according to their means and their sympathy with our object. Of the 10,000*l.* required, we have not yet obtained half.

E. RAY LANKESTER,
Secretary (*ad interim*)

11, Wellington Mansions, North Bank, N.W.

The Equatorial Coudé of the Paris Observatory

IN continuation of my first letter I now proceed to answer M. Lœwy's second letter, as published in your issue of May 15 (p. 52).

M. Lœwy has not, as I said in that former letter, raised a single objection which had not already been anticipated and discussed with the exception of one which I shall treat of further on. The several points in this letter I shall dispose of very shortly.

1. As to the dialyte construction, I have to reply that that particular method of achromatising the objective is not an *essential* feature of this instrument. Whether it be adopted or not is in fact much a question of *cost*. If the purchaser desires to get the largest possible aperture at least expense, then I would make it a dialyte, for, notwithstanding all M. Lœwy says, good work can be and has been done with dialytes. If, however, the most perfect instrument is desired, I would dispense with the dialyte construction, and achromatise the object-glass in the ordinary way, which is quite as applicable to my construction of equatorial as is the dialyte. If I mistake not, the celebrated observer M. Dembowski observed for many years with a dialyte, and spoke highly of it; he says: "*L'achromatisme est excellent.*" Again, the present director of the observatory, for whom the first of these instruments is to be made, has worked already with dialytes, and he would not be likely to recommend this construction if his experience agreed with M. Lœwy's. I desire to notice just one further point in this part of M. Lœwy's letter, as it is another example of how his own words (unintentionally, no doubt) confirm my statements. He says (speaking of the limited field of view of dialytes): "But, in order to turn the difficulty, he" (Mr. G.) "suggests that since the field of view becomes smaller as the instrument becomes larger, we may content ourselves with observing at a central point." I never said this; my words were: "The definition at the edge of the field, however, is not so good as in the ordinary form, but this would not be of so much consequence in large instruments, as the field in such cases is never of great extent." And M. Lœwy himself corroborates this for me when he says: "For the observation of comets I have such an eyepiece, which magnifies fifty times, and has a field of view such that I can observe a degree (*i.e.* with the 12″ equatorial *coudé*); for a telescope of 27 inches we might have such an eyepiece with a field of 24 minutes." Thus I have a distinct corroboration from M. Lœwy of what I said above.

2. Writing on the matter of stability, M. Lœwy curiously mixes up stability and accuracy of movement. Now while I claim that I can and will obtain greater stability in my form than exists in M. Lœwy's, I do not claim accuracy of movement, but on this point I propose to say very little at present for several reasons. In the first place, it would hardly be possible to discuss this and put it in an intelligible form to your readers without a careful drawing; secondly, the well-known stability of the instruments which have emanated from my workshops are quite sufficient guarantee that this point is not one likely to be neglected in any of my work; and thirdly, I find it utterly impossible to understand the sentences of M. Lœwy's paper bearing on this point, and if I, though familiar with the proposed construction, fail to understand them, I am hopeless of serving any useful purpose by discussing them in your columns, particularly as few of your readers have ever seen the design of the instrument referred to. M. Lœwy talks of "all movements of transmission being broken at right angles." I do not know what he means, but he omits to tell your readers that, according to my design, in the larger sizes I propose that all movements be effected by two hydraulic cylinders the valves of which are within reach of the observer while sitting in his chair; so that, without more physical exertion than is necessary to open a water-tap, he has full command of all the movements of the great instrument, a pair of vertical scales on the walls of his study giving the approximate position of instrument in R and declination, an arrangement eminently calculated to reduce the work of the observer.

3. Lastly, as to its want of universality. This is distinctly stated in my paper as a disadvantage of my form; but when M. Lœwy asserts that "it is based on a principle which no astronomer can admit, viz. that it is superfluous to observe the greater part of the northern heavens," it is evident that M. Lœwy has here gone too far, since that portion of the heavens within 20° of the Pole is only about 6 per cent. of the northern hemisphere. Ask any practical astronomer possessing a moderate-sized equatorial how many hours out of the total number of hours which he has worked in the year has his instrument been pointed to objects within 20° of the Pole, and, with the exception of a few who apply themselves to special work, the great majority will give a reply which will show how very little will be lost by the fact that this instrument cannot command that portion of the heavens. I have myself put this question to many, and with the result above mentioned. On this point I cannot do better perhaps than give an extract from a letter I have just received from the director of one of our public observatories:—"Instruments of large aperture are rarely if ever used for observations where extreme accuracy of measurements is required, such as annual parallax, nor for searching for nor observing comets, except to search along a known track for an expected periodical comet. This your instrument could do well. There is hardly an instrument in existence which is equally well adapted to all kinds of observations. The circumpolar zone of about 20° may be explored by other instruments, but for almost every kind of *systematic* work the remainder of the visible heavens will give plenty to do." The foregoing would be a sufficient answer to a question which M. Lœwy has put directly to me.

He says:—"Permit me to ask Mr. Grubb how he is going to study that part of the heavens which lies between 20° from the zenith and the Pole."

To any one who has seen my paper it will be evident that this point, which in M. Lœwy's letter is put forward as a discovery of his own, was already fully dealt with by me. I said: "The instrument commands the heavens from east to west and from south horizon to about 20° beyond zenith." And again: "As regards this instrument (equatorial *coudé*) I would observe that it possibly possesses an advantage over my form in being absolutely universal."

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No doubt universality unaccompanied by such disadvantages as the equatorial *coudé* possesses is very much to be desired, but I do not yet see how it is to be obtained. With respect to the solitary original objection that M. Lœwy has raised, viz. that the light reflected by the mirror varies with the different angles of inclination, and therefore renders the instrument unfit for photometric researches, I confess this objection did not occur to me before, and I am inclined to think M. Lœwy is right, and that my instrument will not be well adapted for photometric researches, but I ask: Is the equatorial *coudé* any better? On consideration it will be evident to your scientific readers that the light after the first reflection is elliptically polarised, and if so the quantity reflected by the second mirror is variable at the various angles of declination; consequently photometric observations made with the equatorial *coudé* cannot be relied upon.

In attempting to prove his case M. Lœwy gives in his first letter a considerable number of numerical details, and no doubt most of your readers have taken these figures as correct. I will ask them, however, to verify for themselves a few of them, and the result will, I think, show how very loosely M. Lœwy has put these data together. For instance, he mentions the weight of a 40-inch mirror, whose thickness is just one-sixth of the diameter, to be 380 kilos., and he calculates (see further down) that a mirror of 38 inches diameter and proportional thickness would weigh 280 kilos.—100 kilos. less. Now, if the thickness be proportional, the weight should be as the cube of the diameter. If your readers will try this themselves, they will find that M. Lœwy has in this case exaggerated the difference to the amount of about 100 per cent.

I find I omitted to notice just one point in Mr. Lœwy's first letter. He says:—"If Mr. Grubb had looked at the drawing which I published in the *Journal de Physique* of last year, he would have seen that it is almost identical with that which he has communicated to the Royal Dublin Society, so far as the general arrangements for sheltering the observer and instrument are concerned."

Permit me to inform M. Lœwy that this would hardly have been a novelty to me last year, inasmuch as I had such arrangements not only on paper but in actual work for some years back, and a description of the same was published in the Royal Dublin Society's *Proceedings*, April 1879.

Your readers will see from the foregoing that M. Lœwy's whole letter is based on a series of misconceptions of statements in my paper in the *Transactions* of the Royal Dublin Society.

Some of the mistakes that M. Lœwy has fallen into were perhaps due to the fact that the plate issued with the Royal Dublin Society's *Transactions* was merely a diagram without details, introduced to illustrate the principle of the mounting. He assumes that details not figured in the diagram are not to be provided, in spite of the fact that in the text of my paper I discussed several of them.

M. Lœwy occupies nearly half a column of NATURE in speaking of the labour involved in working this instrument, because no tube is shown in the diagram connecting the equatorial part with the ocular; all this trouble would have been saved if he had read my paper a little more carefully, for then he would have found that not only did I say, "In most cases it would be desirable to have a connecting tube," but I even discuss the best form of tube for the purpose. There are some special cases in which a tube would not be actually necessary.

It appears to me that M. Lœwy is very unnecessarily disturbed in his mind by the advent of my instrument. No doubt the equatorial *coudé* and my siderostatic telescope have each their own sphere of work, and there may be room for both. An observatory having at its back a generous individual who (as was stated at a late meeting of the Royal Astronomical Society), has already expended a quarter of a million on astronomical observatories and is willing to spend more, can afford a large instrument perhaps on M. Lœwy's plan; but as all observatories are not equally fortunate, there may occasionally be one found which will be glad to get equally great optical power at one-third the cost.

I barely alluded in my last letter to this question of cost. On this point it may be desirable to supplement what I have said in my former letter, bearing in mind that the cost of the instrument will, as I have above stated, depend somewhat on whether or not the objective is achromatised on the ordinary principle or that of the dialyte.

In order to put the matter of cost in the clearest light, let us consider the four forms which we have at present to select from, viz. the ordinary equatorial, M. Lœwy's equatorial *coudé*, my siderostatic telescope with objective achromatised in the ordinary way, and the same instrument with objective achromatised on the dialyte principle.

Let us consider first what apertures we can obtain in the several forms for a given sum; assuming M. Lœwy's figures for the equatorial *coudé*. For 1760*l.* can be obtained—

(*a*) Equatorial *coudé* of 12-inch aperture.

(*b*) Ordinary equatorial of 12-inch aperture, including its dome and observatory.

(*c*) Siderostatic telescope with objective achromatised in the ordinary way of 18 inches aperture.

(*d*) Siderostatic telescope with objective achromatised on the dialyte principle of 24 inches aperture.

It would be for the astronomer to say whether the double aperture of the objective would not more than counterbalance the disadvantages of want of absolute universality.

Let us, secondly, consider for what prices the *same* aperture could be obtained in the various forms:—

(*a*) Equatorial *coudé* 12″ aperture	£1760
(*b*) Ordinary equatorial of 12″ aperture, including dome and observatory	1760
(*c*) Siderostatic telescope of 12″ aperture with objective achromatised in the ordinary way ...	1000
(*d*) Siderostatic telescope of 12″ aperture, with objective achromatised on the dialyte principle	500

The difference between cost of equatorial *coudé* and siderostatic dialyte (about 1200*l.* for this size) will probably be considered by the purchaser rather too large a sum to pay for the possibility of examining the 6 per cent. of the northern hemisphere which is beyond the reach of my siderostatic telescope, particularly when it is borne in mind that that portion is the least important part of the heavens.

M. Lœwy does not say whether the 1760*l.* includes cost of observing hut. If not, the comparison is still more striking, for, although the equatorial *coudé* requires a special building, my siderostatic telescope does not. HOWARD GRUBB

Dublin, May 27

The Earthquake

CHANCE brought me to Colchester about a week after the earthquake, and since then I have been amusing myself mapping the effects of it, and hope to read a paper on the subject at the meeting of the Royal Geological Society, Ireland, next month. In the meantime I would like to draw attention to a few of the general facts that seem not to be recorded.

The area of structural damage lies at and southward of Colchester, principally to the west of the Colne estuary, and in it there are five smaller areas in which are found the greatest damage. These areas occur in the following order:—Wivenhoe, Peldon, Abberton and Langenhoe, Colchester, and West Mersea; each of these have two or more well-marked margins; where these margins can be easily studied, there are found to be lines of breaks, and alongside one, or in places two of them, the greatest destruction occurred, while at the other side of such lines the damage is a bagatelle in comparison.

Thus at Wivenhoe, where there was the greatest damage done, the shock came from the north-east; but when it reached the break of the Colne River valley, it seems to have recoiled as if from a percussion blow. Westward of the estuary of the Colne the damage at Rowhedge was slight when compared with that at Wivenhoe, while at Hornwood it was still slighter, although the last is only divided from Wivenhoe and Rowhedge by the valleys of the Colne and Roman Rivers.

At Peldon, where the shock appears to have been nearly as bad as at Wivenhoe, the damaged area is very well defined, being bounded northward and southward by stream valleys. The shock seems to have travelled southward and to have recoiled from the southern boundary, causing excessive damage alongside it. Here also the shock appears to have had a rotary motion, which possibly may be due to the recoil against the southern boundary.

In the Abberton and Langenhoe area the shocks seem also to have had a rotary motion, the main direction seems to have been from the south-east; here the greatest damage occurs at the western boundary.

At Colchester the shock was going north, while at West Mersea it went south. In both of these places the boundaries of the areas are in part obscure. In the first, however, we can trace the tract of maximum damage from Head Gate along the south Roman wall and eastward to Colne valley, east of which very

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